wires, and all difficulty about hearing is overcome. I hope I shall shortly have the most perfect clock in England, perhaps in the world.

Of the principal instrument I have to announce that I have decided on having an altazimuth, but constructed on a new principle. I have fixed on Steinheil's principle of making the horizontal axis the effective optical axis, by placing the object-glass at one end, and the eye-piece at the other, with a prism outside the object-glass. The casting of the prism took over three months, but Messrs. Chance succeeded at last; six inches aperture is adopted, and six feet three inches focal length. It is some comfort to think that never need the telescope be raised, only turned round, and the observer always under cover. The principal vertical axis is steel, and the bed-plate of cast-iron. The telescope-tube is also cast-iron. The circles are both of thirty inches diameter; the altitude-circle of gun-metal solid throughout, that is without spokes, and cut out from a much larger piece. The microscopes are of five feet focal length, and I hope to try photography with them.

On certain important conclusions deducible from the Observations made on the Transit of Mercury at Greenwich, on November 5th, 1868. By Richard A. Proctor, B.A.

From a careful study of the investigations to which the observations made on the transit of *Venus* in 1769 have been subjected, and more especially of the masterly researches of Mr. Stone in the *Notices* for October 1868, I came long since to the conclusion that one of the chief points to be considered by astronomers in preparing for the coming transits is the effect which has been termed the "clinging of the limbs of the Sun and planet" near the time of true internal contact. In an article which appeared in the *Daily News* of November 4, 1868, I called special attention to the advantages which might result to science if observations were made on the internal contact of *Mercury* with reference to this effect. I wrote as follows: "Though transits of *Mercury* are not in themselves very important phenomena, it cannot be doubted that astronomers will avail themselves of the opportunity to practise, so to speak, for the approaching and far more important transits of *Venus* in 1874 and 1882. They will inquire whether the magnifying power of the telescope made use of has any bearing upon the duration of the deceptive appearance, or whether darkening glasses somewhat more powerful than those usually employed may not diminish the irradiation to which the phenomenon is due." A week later I wrote a letter to the same effect to the editor of the *Scientific Opinion*, and in a paper which appeared in that journal yet a week later (but was

in fact written at the same time as the letter), I called particular attention—first to the bearing of Mr. Stone's re-investigation of the transit-observations of 1769 on the subject of the coming transits; and, secondly, to the probable value of a "well-concerted plan of observations or even of a comparison, *inter se*, of observations already made on the peculiarity in question."

I mention these facts to explain the interest I take in the valuable series of observations made at Greenwich on the transit of *Mercury* in 1868, and particularly in the examination to which Mr. Stone subjected those observations. Certain highly important consequences seemed to me so obviously to flow from Mr. Stone's paper on the subject in the *Monthly Notices* for November, 1868, that in all cases where in treating the coming transits of *Venus* these consequences have been in question, I have in effect taken them for granted, not wishing to waste space in pointing out what Mr. Stone had already, as I conceived, demonstrated.

But as I judge from a passage in Mr. Stone's last communication to the Society that he himself reads his observations differently than I had been disposed to do, I feel that the importance of the whole subject will justify me in inviting attention to the conclusions which may, as I conceive, be directly deduced from Mr. Stone's "Remarks and Suggestions arising from the observations of the Transit of *Mercury* across the Disc of the Sun." (*Monthly Notices* for November, 1868.)

I refer now principally to the remark in his last paper, that the errors of contact-observations, "arise when we assume that the phase of the phenomenon observed by one observer corresponds to the phase observed by another observer in such a way that each takes place at the same distance between the centres." I had so read Mr. Stone's earlier paper as to believe its main object was to *prevent* observers from falling into this sort of error; and I still, after renewed and careful study, not only of that paper, but of the whole subject, can form no other opinion than that the practical value of Mr. Stone's researches is altogether greater than he seems to believe. Instead of looking upon his remarks as merely suggesting a difficulty, I find in them the clear indication of a method of escape from that difficulty.

To save space, I will take only the extreme cases of difference referred to in Mr. Stone's last paper.

Mr. Lynn, observing with the north equatoreal and power about 170, saw a delicate filament form itself between *Mercury* and the Sun's limb at $21^h\ 0^m\ 0^s{\cdot}7$ G.M.T. The filament was so fine that we may believe that Mr. Lynn really "caught a phase very near that of real internal contact."

Mr. H. Carpenter, using the east equatoreal and a power of 70, observed the sudden formation of a broad ligament between *Mercury* and the Sun at $21^h\ 0^m\ 11^s{\cdot}1$ G.M.T.; and on Mr. Stone's requesting him to point out which of certain figures (see *Monthly Notices* for November 1868) "appeared to him to represent most

nearly the appearance of the planet at the time of his observation, he without hesitation pointed out fig. (3)," but the phase, Mr. Stone says, was "somewhere between (2) and (3)."

Now in fig. 2 of the above-named paper, the ligament is rather less than one-half the diameter of *Mercury* in width, in fig. 3 rather more than one-half. We may suppose that in reality it was as nearly as possible one-half.

Therefore, supposing we wished to determine the time of real internal contact from Mr. Carpenter's observation, we ought, I apprehend, to proceed as follows.

We may assume, first of all, that irradiation does not tend to make the cusps seem appreciably nearer together than they actually are, because on no other hypothesis can we account for their squareness of outline. It follows, then, that the chord joining the cusps at the time of Mr. Carpenter's observation was equal in length to about one-half the diameter of *Mercury*. Hence it subtended at *Mercury's* centre an angle of about 60°, and therefore *Mercury* had really crossed the Sun's limb (normally) by a distance equal to *Venus'* radius × vers. 30°, approximately.

Now in the transit of Nov. 1868, *Mercury* traversed an arc of 41°, from external contact to external contact (Sun's semi-diameter $16' \, 10''\cdot 7$, *Mercury's* $4''\cdot 9$) in $3^h \, 38^m \, 18^s$. And it is easy to calculate from this *Mercury's* normal velocity, which I find was $0''\cdot 06412$ per second. Hence the interval which had elapsed between real contact and the time when Mr. H. Carpenter observed the formation of the broad ligament, was

$$= \frac{4\cdot 9 \;.\; \text{vers}\; 30^\circ}{0\cdot 06412} \text{ seconds of time,}$$

$$= 10\cdot 3 \text{ seconds.}$$

Hence the calculated epoch of real internal contact is—

$$21^h \, 0^m \, 11\cdot 1^s \text{ G.M.T.} - 10^s\cdot 3$$
$$= 21^h \, 0^m \, 0^s\cdot 8 \text{ G.M.T.}$$

Mr. Lynn's observation gives, for the same phase—

$$21^h \, 0^m \, 0^s\cdot 7$$

Hence, when thus treated (and there is nothing forced in the above method, nothing, in fact, which is not strictly accordant with Mr. Stone's interpretation of the nature of the clinging), Mr. Carpenter's observation gives a result *differing by only one-tenth of a second from Mr. Lynn's.*

I do not suppose for a moment that the closeness of agreement in this particular case is not in part accidental. But it appears to show very clearly that we can, by treating in the same way observations made on *Venus* in 1874 and 1882, obtain results *much* more accurate than by neglecting the considerations flowing from Mr. Stone's remarks on the transit of *Mercury*.

Of course, if those who come to manipulate the observations of

1874 and 1882 should insist on assuming that a phase caught by one observer is identical with a different phase caught by another, the old cause of errors will creep in. But in doing so they would be blinding themselves to the real value and significance of the observations made at Greenwich on the morning of November 5, 1868. This will not happen I should imagine. If, however, an Encke of the future should make such a mistake, doubtless there will not be wanting a Stone of the future to correct the results so obtained.

So long as the observers in 1874 and 1882 indicate as closely as they can the apparent breadth of the dark ligament, it must always be possible to determine the moment of real contact much more exactly in the manner above indicated than by assuming the phase observed to be actually a contact. It is for this reason that I have been always unwilling to accept the view that the probable error will be proportional to the slowness of the planet's normal velocity. I believe, on the contrary, that, during a transit of slow normal velocity, the time given to the observer to form several estimates of the breadth of the ligament, will counterbalance (not wholly, but to an important extent) the absolutely greater time-intervals which separate different phases of the internal contact. I have not before entered at length into the reasons which led me to this view, because they seemed so clearly deducible from Mr. Stone's valuable remarks on the transit of *Mercury*.

In the preceding paragraphs I have referred only to ordinary eye-estimates of the breadth of the dark ligament. It seems so obviously suggested that the eye-piece of each telescope made use of should be provided with an arrangement for estimating the breadth of the ligament — as compared with the diameter of *Venus*, that perhaps it may seem unnecessary that I should mention the point. Still, as I have found hitherto no reference to the necessity of this being attended to, I may be excused for remarking on it.

Micrometrical measurement is, perhaps, not desirable, though it would clearly be possible to apply it in an effective manner. It is not necessary, however, because absolute dimensions are not required. All we want is to know the ratio which the breadth of the ligament bears to the diameter of *Venus*. Hence if we adopt any of the numerous contrivances by which a series of cross-lines (or two series at right angles to each other would be even better) can be made to appear in the field of view with *Venus*, it will be the simplest possible matter for the observer to determine the relative breadth of the ligament — and this, even though at the moment of contact neither set of cross-lines should be absolutely normal to the Sun's limb at the place of contact.

If it should be thought advisable (as was suggested in November 1868 by the Astronomer Royal) that places of observation should be so selected that *Venus* will cross the Sun's limb either near its highest or near its lowest point, all that is requisite is,

that the place of observation should be taken as near as possible to the curved diametral arrow which lies across my two charts of the Earth in the *Monthly Notices* for June 1869 (plates 5 and 6). Most of the stations already dealt with are little affected by this condition; but, for observing retarded egress, all the Indian stations will be found (in this respect) far better than Alexandria. At Peshawur, for instance (a place already superior as respects coefficient of parallax and solar elevation), *Venus* will leave the Sun almost exactly at his uppermost point, whereas at Alexandria the point of last contact will be about 34° from the uppermost point of the Sun's limb.

A New Theory of the Milky Way. By R. A. Proctor, B.A.

Sir W. Herschel's respect for existing analogies—a quality which is perhaps of all others the safest guide for the scientific explorer,—led him to adopt as the means of interpreting his noble series of star-gaugings the hypothesis that there is a general uniformity in the distribution of the stars through space. He adopted this hypothesis not from a conviction of its being actually true, nor even from the belief that it is approximately so, but simply because existing analogies seemed to render it probable, and because it formed a convenient basis for calculation. The existing analogies were those presented by the solar system. In this system, Sir W. Herschel recognised a number of discrete bodies, not equal indeed, but comparable *inter se* in magnitude; not uniformly distributed, but still not aggregated towards one or another part of the solar scheme. And making such modifications as seemed requisite in comparing a system not regulated by a vast central orb with a scheme like our solar system, it seemed likely to him that a general equality of magnitude, and a general uniformity of distribution, might be found to prevail among the members of the sidereal system.

We now know that the ideas which astronomers had formed of the solar system in Sir W. Herschel's day were very far indeed from being correct. We see in the solar system a complexity of detail, and a variety of form, structure, aggregation, and motion, which were altogether unknown a century ago. And I cannot doubt that if the view we have of the solar system had been presented to Sir W. Herschel, he would have adopted as the basis of his star-gaugings an hypothesis differing altogether from that of which he actually availed himself. He would have argued, that as, in the solar system, there are bodies like the planets, far surpassing the other members of the scheme in magnitude and in importance; as it contains zones of minute bodies, such as the asteroids and the satellites composing the rings of *Saturn*; myriads of meteoric systems, and countless thousands of cometic systems, so doubtless in the sidereal system there are many forms of matter. If the analogy of the solar system is to be our guide, we